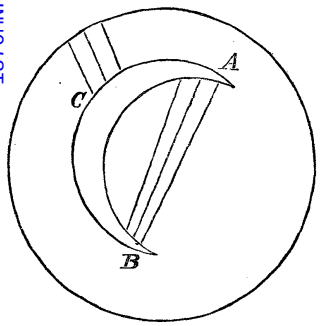
twenty-five minutes before totality, Prof. Zentmayer observed some bright objects on the ground glass, crossing from one cusp to the other of the solar crescent, as indicated by the accompanying



cut by the lines from A to B; each object occupied about two seconds in passing, and they all moved in right lines, nearly parallel, and in the same direction. These points were well defined, and whatever they might be, must (in order to produce such sharply defined images on the ground glass) have been several miles distant from the telescope. After calling Prof. Himes' attention to this phenomenon, and observing some eight or ten bodies in all, Mr. Zentmayer then noticed three others coming in from the limit of the field, and disappearing in the solar crescent, as shown at C; but not reappearing on the other side. It is worthy of note that the direction of motion of the three bodies last mentioned coincided with that of the wind blowing at the time, but that of the others did not; and they are thus, as also for other reasons, unlike the plant seed noticed some years ago by Mr. Dawes."* Prof. Coffin also saw "meteoric bodies cross the telescope from east to west like bright flakes." †

The eclipse having taken place very near the periodical date of the 10th of August, it is interesting to remark that at Vevay, Indiana, during the eclipse, several meteors were seen, at an altitude of about 45°, taking a westerly direction. On the other hand the bright objects seen near the Sun's disk by Prof. Zentmayer, and the bright flakes noticed by Prof. Coffin, were not impossibly caused by the distant passage between the observer and the Sun's disk of some winged tribe as the extraordinary flight of locusts, seen through the telescope by Lieut. Herschel, in India, in October last.

Notes on the Solar Corona and the Zodiacal Light; with suggestions respecting Observations to be made on the Total Solar Eclipse of December 24th, 1870. By Richard A. Proctor, B.A.

The total eclipse of next December will last so short a time that, if possible, no part of that time should be wasted through a misapprehension of the nature of the phenomena to be observed. On this account I cannot but think it would be a matter to be much regretted that mistaken views should be promulgated respecting the corona, supposing it to be possible,—which I take

* *Monthly Notices*, for 1852, p. 183.
† *Journal of the Franklin Institute*, for 1869, p. 152.

to be the case,—to form just views from the evidence already in our hands.

The principal object of the observations to be made next December will be to ascertain the characteristics of the solar corona. Observers will certainly be able to work much more effectually if they know beforehand the general nature of the phenomenon, for they will thus be guided not only in the selection of modes of observations, but also by knowing what points it is most important they should attend to.

I think it so essential to avoid raising unnecessary doubts, that I would not venture to express the opinion that the corona is wholly a solar appendage if I had not given the matter very careful consideration, and found the evidence overwhelmingly strong in favour of this view.

It is hardly necessary to discuss the theory that the corona is due to the diffraction of solar rays which pass near the Moon's edge, because that theory has been thoroughly disposed of by Brewster's arguments. Nor need we consider La Hire's theory that the phenomenon is due to the reflection of the solar rays from the irregularities of the Moon's surface, as it is obviously inconsistent with the observed peculiarities of the corona.

But a theory has recently been put forward that the corona is simply due to the glare of the terrestrial atmosphere, and this theory has been adopted by astronomers of standing. I hold it to be important, therefore, that this theory should be subject to careful scrutiny, as undoubtedly, if it be erroneous, much mischief may be done to the cause of scientific progress by its promulgation at the present conjuncture.

The first and most obvious evidence against this theory is the fact that the Moon is projected as a dark disk on the bright background (so to speak) of the corona. The theory requires that the corona should, in fact, not be a background, but a foreground; and one might naturally inquire how the Moon which is beyond the Earth's atmosphere should come to be apparently projected upon the supposed glare of that atmosphere.

But though this circumstance is in itself decisive of the matter at issue, let us turn to less obvious considerations. As a matter of fact, we know that light reaches the eye along lines tending from the neighbourhood of the eclipsed Sun. Let us inquire whether in those directions there is illuminated air; if not, optical considerations will force us to regard the source of light as beyond the air.

The eclipse of next December is not a favourable one for my argument; but it will be more interesting, and perhaps more useful, to consider it than any other.

In fig. 1, let A represent the position of an observer on the line of central eclipse, somewhere in the south of Spain. At such a station the eclipsed Sun will be almost 30 degrees above the horizon; and I find from a valuable paper which Mr. Hind has been good enough to forward to me, that the shadow-cone will

be about 50 miles across, where it reaches the Earth. Obviously, then, the shadow on the Earth will be an ellipse whose major axis will be about 100 miles, its minor about 50 miles in length. Let A S then be drawn inclined at an angle of about 30° to H A H′, the horizon line at A, in a vertical plane through the Sun; and, having A *c*, A *c′* each to represent a space of 50 miles, let *c m* and *c′ m′* be drawn, each inclined about 16′ to A *c*,

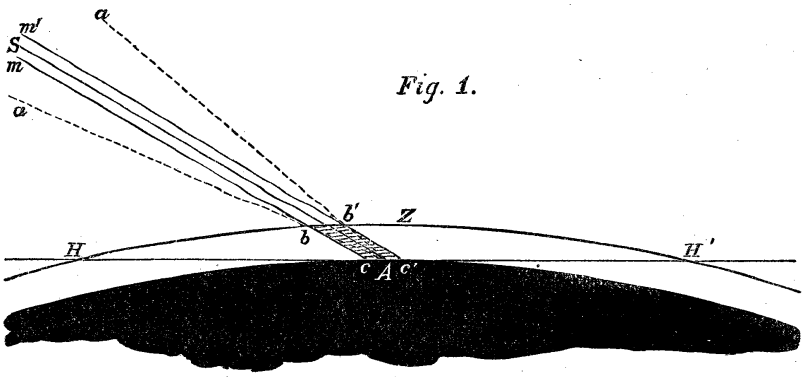


Fig. 1.

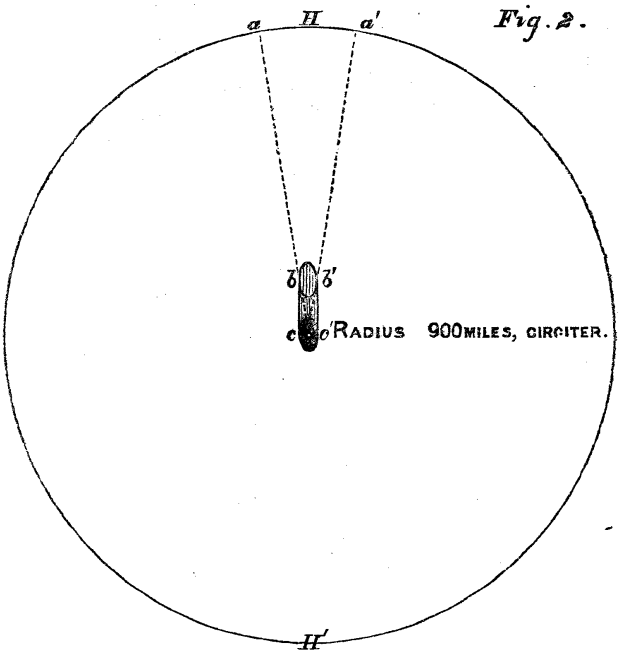


Fig. 2.

so that while A *c* is directed towards the Sun, *c′ m′* and *c m* would be directed towards the highest and lowest points of the Moon's limb. Then *m′ c′ m c* is a vertical section of the Moon's shadow.

Now we do not know the height of the terrestrial atmosphere, but we may confidently believe that no air above the height of 100 miles can reflect any appreciable amount of solar light to

us.* Let us therefore take A Z to represent 100 miles; then H Z H′ will represent limits of the light-reflecting air, where H H′ is about 18 times as great as A Z. The portion of the atmosphere above the horizon-plane of the observer will therefore be of the figure produced by the revolution of H Z H′, about the vertical axis A Z. It will be, in fact, a plano-convex lens.

Let *c m* and *c′ m′* meet H Z H′ in *b* and *b′*; then the portion *b c c′ b′* will be in the Moon's shadow. (The effects of refraction are obviously insignificant.) The only light which can reach this part of the atmosphere is that from the chromosphere (to use a convenient but unsatisfactory name) and the coloured prominences, or from the earth and surrounding illuminated air. Towards *b* and *b′* the observer will recognise the first faint traces of directly illuminated atmosphere, and the light will gradually increase above *b′* and below *b* (more rapidly in the latter case than in the former). By a careful construction (a method quite exact enough for such an inquiry as the present), I make the the angle S A *a* about 6°, and the angle S A *a′* about 9°.

This, however, refers to only one section of the shadow-cone. To determine (roughly) the extent of illuminated atmosphere in a horizontal direction, we have only to consider the air-lens H Z H′ as supposed to be viewed from above. In fig. 2, *c c′* represents the actual shadow on the earth; *b b′* the intersection of the shadow-cone with the limits of our hypothetical envelope 100 miles high. Thus *b b′ c′c*, fig. 2, represents simply a vertical view of the portion *b c c′ b′* of the shadow-cone in fig. 1. Lines *b a*, *b′ a′*, drawn from the centre of the ellipse *c c′*, touching the ellipse *b b′*, give approximately the angular width of that part of the heavens within which no atmosphere directly illuminated by the Sun can be visible. I find from a careful construction that *a b* and *a′ b′* would include an angle of about $14\frac{1}{2}°$.

Thus we obtain a nearly circular region (in which the Sun is eccentrically situated), having a horizontal diameter of about $14\frac{1}{2}°$, and a vertical one of about 15°, within which there is not any light whatever from directly illuminated air. The Sun would be about 6° from the lowest point of this dark region.

With regard to the light from the prominences and the chromosphere, upon the air within this region, we know that it cannot suffice to light up the air with any strong, if even with

* Bravais, from a discussion of Lambert's observations of the crepuscular curve, deduced a height of nearly 100 miles. His own observations, made from the summit of the Faulhorn, gave a height of about 66 miles. Neither estimate refers to the actual limits of the atmosphere however. Dr. Balfour Stewart considers that perhaps the best means of judging on this point would be by observations made on the aurora. From such observations made in 1819, Dalton estimated the extreme height of the auroral light at 102 miles; Sir John Herschel estimated the height of an auroral arch seen on March 9, 1861, at 83 miles (undoubtedly the aurora is often seen much lower). The limits of air capable of reflecting light, must certainly lie much below the actual limits of the terrestrial atmosphere.

any appreciable glow; because we know how small a relation ordinary atmospheric glare bears to direct solar light, and the glare due to the chromosphere and prominences would bear a similar relation to the direct light from those sources. But further, whatever light came in this way, would obviously illumine the outer parts of the shadow-frustum $b\,b'\,c'\,c$ more strongly than the parts near the axial line A S. Hence a faint diffused light diminishing *towards* the neighbourhood of the Moon should result.

As regards the illumination of our shadow-frustum by light derived from the neighbouring illuminated atmosphere and from the Earth, it is only necessary to remark that even when there is no eclipse the light thus falling on such a region as $b\,b'\,c'\,c$ would be small; but that while a total eclipse is in progress all the parts near the shadow-cone are in nearly total eclipse, and not any part of the whole region H Z H′ is illuminated by so much as half the solar disk. Further, the light derived from this source, like that derived from the prominences and chromosphere, should diminish towards the neighbourhood of the Moon's disk, instead of increasing as the coronal light does. Also, the light from all these sources should extend over the Moon's disk, since it would illuminate the air between the observer and the Moon's body.

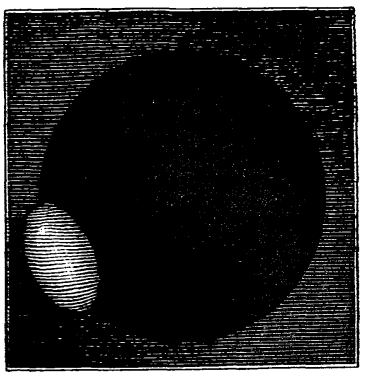
It follows, then, that so far from giving an account of the corona, atmospheric glare gives us a dark region round the eclipsed Sun, and a gradual increase of light with distance from him.

Within this dark space the disk of the Moon, illuminated by the Earth with about thirteen times as much light as the new Moon sends to us, ought to be conspicuous by its relative brightness.

Now, though the reasoning here deals with relations so simple that a mistake can hardly arise, yet there are certain tests to which these conclusions may be submitted before we proceed.

It is clear from fig. 1, that *before* the limits of the total shadow reached A there *should* be atmospheric glare towards the Sun, and further that this glare should at first wholly cover the Moon, and rapidly sweeping across her disk, just before totality, should pass away from her neighbourhood with undiminished velocity. It would be difficult to detect such a phenomenon by ordinary observation; though, as I shall presently show, not impracticable. But supposing a photograph could be taken an instant before totality, we might catch the glare while in the act of crossing the Moon's disk. Now this could only be managed by a miracle of dexterity; but by a miracle of good fortune, it *has* been managed already. The first photograph of Lieut.-Col. Tennant's admirable series was taken an instant before totality commenced; and *there we have the glare just about to leave the Moon's disk,*

Fig. 3.



but still trenching most obviously upon it. Fig. 3 represents the feature here dealt with. The light here is true atmospheric glare, and we see that, as might have been thought obvious, it is not limited by the Moon's disk which lies so far beyond the limits of the air. Then also we notice another important point. The edge of the glare is obviously travelling much faster than the Moon; for while the Moon proceeds to obliterate the last remaining point of the Sun's disk, the glare traverses the much wider distances separating its inner edge from the Moon's limb. Clearly this velocity would carry the glare clean away from the Moon, as the above reasoning shows should be the case

Again, it will be obvious, from a study of figs. 1 and 2, that during an annular eclipse, at the moment when the shadow-cone is pointing directly, or almost directly, towards the observer, the centre of the Moon's disk ought to be much darker than the edge. Now in the 10th volume of our *Memoirs*, Mr. Baily states that, while observing the eclipse of 1836, he noticed, on looking at the Moon through a telescope during the annularity, that "the circumference was tinged with a reddish purple colour which extended over the whole disk, but increased in density of colour according to the proximity to the centre, so as to be in that part nearly black." It is obvious that this appearance could last but a few seconds, since the moment the axis of the shadow-cone was turned appreciably away from the observer (and the vertex of the cone travels fully 20 miles per minute), he would be looking through the cone's *sides*. The following passage from Klein's *Sonnensystem* describes the whole phenomenon precisely in accordance with this view: "Bei der ringförmigen Finsterniss, am 30 October, 1864, sah Mouchez zu San Catharina in Brasilien, im Augenblicke als die Scheiben von Sonne und Mond concentrisch waren, das Centrum des Mondes völlig dunkel, aber von hieraus gegen dem Rand nahm die Helligkeit regelmässig zu und letzterer erschien heller, oder doch wenigstens ebenso hell, als das aschgraue Licht der Mondsichel, kurze Zeit vor oder nach dem Neumonde. Die ganze Erscheinung verschwand und die Mondscheibe war gleichförmig dunkel, als der leuchtende Ring gerissen und die Mitte der Finsterniss vorüber war."

Taking the corona to be a solar appendage, it is clear that even in total eclipses a somewhat similar appearance might be looked for, the outer parts of the Moon's disk during central totality seeming brighter than the centre, because the atmosphere between us and those parts would be more fully lighted up by the corona. I find, accordingly, that M. Tissel, observing the

total eclipse of 1733 at Skepshat, in Sweden, saw the Moon's surface brighter at the margin, and black towards the middle. We see from this most clearly that the atmospheric glare in this region is very much fainter than the corona, for except on a very close examination the Moon's disk, though the glare appears over a part of it during the totality, seems absolutely black, and is so rendered in photographs.

But further, if the views expressed above are correct, it ought to be possible under favourable circumstances to see the Moon's face by reflected Earth-light. I find that Bigerus Vassenius, during the remarkable total eclipse of 1733, using a telescope of 21 feet focal length, perceived the principal spots on the Moon during the total obscuration (*Phil. Trans.* 1733, p. 135). Ferrer also saw the spots on the Moon's surface very plainly during the total solar eclipse of 1806.

Yet again, if the apparent blackness of the Moon's disk results from the fact that the coronal light is beyond the Moon, and so forms the background on which she is projected,* two phenomena might be expected to be visible under favourable circumstances. First, the entire outline of the Moon's disk ought to be visible in partial eclipses, or before and after totality; and secondly, the corona ought to be visible at such times, and also, during annular eclipses. I find that the former phenomenon which corresponds in reality to the visibility of the corona (since were there no corona the Moon's limb could not appear dark where it crossed the Sun), has been frequently noticed; it has, in fact, been as often recognised as looked for. The visibility of the corona, when the Sun is not totally eclipsed, has also been so frequently recognised that it is hardly worth while to mention instances in point.† But I may quote, as very remarkable, the the fact that in 1860 Father Secchi saw the corona *for forty seconds after totality was past.* Another remarkable instance is that recorded by M. Edstrom, in the case of the eclipse of 1733, when the unequal radiations of the corona were observed to remain unchanged in position, as they gradually faded out of view with the increasing solar light.

It is further obvious, that if the corona be a solar appendage, one could expect it to appear concentric with the Moon only at

* The fact that the disk of *Venus* appears blacker than the surrounding sky when she is in superior conjunction, can only be explained by supposing there is some light beyond *Venus*. What can that light be but a solar appendage?

† Arago has founded on the visibility of the corona while a portion of the Sun is yet uneclipsed, a calculation of the ratio which the coronal light exceeds that of the atmospheric glare *then* undoubtedly present. That the corona is brighter than the atmospheric glare caused by a portion of the direct solar light undoubtedly follows from the visibility of the corona under such circumstances; but Arago's mode of treating the problem is not exact. He makes the atmospheric glare proportional to the portion of the solar disk visible at the moment. In reality, this proportion does not hold, for the upper regions of the air are illuminated by much more of the Sun's disk at such a time. In fact, the problem is one of much greater complexity than Arago seems to have imagined.

the moment of central eclipse. Now I find numerous instances in which it has been stated that quite obviously the brightest part of the corona was first on the side the Moon had just uncovered before totality, and lastly on the side she was just about to leave uncovered. I also find several statements (one or two very positive) that the corona was centrally disposed round the *Moon* throughout the totality. I would remark on this, that observations of the former kind, besides being more numerous, are severally more effective than observations of the latter kind. For the former refer to the recognition of a phenomenon and afford positive evidence; the latter merely assert the non-recognition of the phenomenon, and supply therefore only negative evidence. The former describe a peculiarity which attracted the notice of observers; the latter may be taken quite as well to indicate a want of skill in observation as the non-appearance of the particular phenomenon in question. All the positive evidence is therefore here also in favour of the view that the corona is a solar appendage.*

It remains that I should touch on other evidence we have of the existence of a solar appendage adequate to produce the observed appearances.

And first let us consider the zodiacal light. We know that even in our latitudes this phenomenon often exhibits a remarkable degree of luminosity towards the horizon and near the core of the gleam (so to speak). But in tropical countries the brightness of the zodiacal light is much more striking, and is seen to grow visibly greater in the Sun's neighbourhood. At heights of from 8000 to 12,000 feet in tropical climates, says Humboldt, the zodiacal light is seen of a brightness exceeding that of the Milky Way between *Aquila* and *Cygnus*. And obviously if we could trace the zodiacal light up to the solar limb, we should see it shining with a glory far exceeding that which it shows even in tropical countries. For we know that the brightest part there seen belongs still but to the outskirts of the object.†

* The centricity of the corona concerns, however, the question whether the corona is a solar or a lunar appendage; since the atmospheric glare should shift even much more obviously with respect to the Moon than a solar appendage would seem to do. No one now holds that the corona is a lunar appendage.

† I ought, perhaps, to show reason for regarding the zodiacal light as a solar appendage, notwithstanding Dr. Balfour Stewart's recent suggestion that it may be a terrestrial phenomenon. But in reality there can be no doubt whatever that the zodiacal light cannot be a phenomenon associated in any way with our atmosphere. Doubtless Dr. Stewart, whose mathematical attainments are well known, must have directed his attention too exclusively to the physical requirements of his theory, or he would not have overlooked obvious mathematical objections against it. The portion of the return-trade region above the horizon of any place is clearly a lamina shaped like a watch glass (slightly convex, see fig. 1), and the whole of this should be illuminated by electrical discharges excited in the way he suggests. We may, in fact, see in this an explanation of the familiar phosphorescence seen sometimes to cover the whole heavens (which gives the same spectrum as the aurora and zodiacal light), and even though at times, or even commonly, only a portion of this lamina should

Hence we should expect to find precisely such a glow of light round the Sun in total solar eclipses as we actually do see.

Again, from what we now know respecting meteors, we may derive abundant evidence in favour of the view that countless myriads of these bodies must always lie in the Sun's neighbourhood. For though, while the meteoric orbits were supposed circular, there was nothing very surprising in the fact that the Earth encountered so many as fifty-six meteor systems (because the zone of such systems seemed to lie close by the Earth's orbit), yet now we know how eccentric the meteoric orbits really are, we recognise the fact that antecedent probabilities would be wholly against the Earth's encountering even one such system, were there not many millions of them. And since she encounters fifty-six at least, we conclude that there must be millions on millions of such systems having their perihelion within the Earth's orbit. These uncounted systems ought to become visible during a total eclipse, since their dispersed members would lie in all directions round the Sun. Those meteoric flights also which were near him (and many must pass very near to him) would shine with a light whose brilliancy would go far to make up for the extreme relative minuteness of the individual meteors. Since near the Sun's disk the line of sight would be directed through a range of many millions of miles over which such meteors must be freely distributed, while along some 200 millions of miles in this direction meteors must be scattered, though more or less sparsely, one can recognise the reason of the brightness of the corona near the chromosphere.

Further, we know from the researches of Leverrier that there must exist continually in the Sun's neighbourhood a quantity of matter sufficiently important to affect the motion of the perihelion of *Mercury*. A few relatively considerable planets (as large say as the asteroids) might effect the observed changes; but far more

be so illuminated, no reason can be shown why that portion should always be an inclined tongue-shaped slip, as it should be to account for the zodiacal light in our latitudes. It is hardly necessary, however, to point out to the astronomical reader that a light which exhibits no parallactic displacement, which varies in position for different latitudes, according to the laws affecting the celestial bodies, which rises and sets according to the same laws, and which lastly effects the neighbourhood of the elliptic, cannot by any possibility belong to the Earth's atmosphere. The zodiacal light *might* be explained as due to a ring of matter surrounding the Earth, at a distance nearly equalling the Moon's, and travelling (as such a ring would) nearly in the plane of the ecliptic. Such an explanation was indeed put forward in 1856 by Prof. Heis. But the phenomena of the zodiacal light are much better explained by the theory that it is due to a solar appendage, even if we admit that the light sometimes extends from the eastern to the western horizon. But while Heis' theory, with overwhelming probabilities against it, has some points in its favour, the theory that the zodiacal light is an atmospheric phenomenon, is absolutely untenable. If anything would render the theory more strikingly opposed to observation than it is, it would be those occasional peculiarities of the zodiacal light which have been thought by some to favour the theory. These peculiarities simply add new difficulties to others already overwhelming.

probably a multitude of minute bodies may be held to be in question. Now the constant presence of meteors in the Sun's neighbourhood would produce the observed results, even though the individual meteors might remain but a brief time in the Sun's neighbourhood, to pass away presently on orbits whose aphelia might lie far beyond the orbits of the major planets.

Further, Mr. Baxendell has shown that certain peculiarities of magnetic and thermal change seem to point very decisively to the existence of a solar appendage holding the position which the corona, regarded as solar, seems to occupy. I have recently had the pleasure of discussing with him many of the relations considered above, and I find that there is nothing in his valuable meteorological researches which opposes itself to that particular view of the corona which I have advocated above, while his main result (which I hold to be of extreme importance) supplies an obvious argument in favour of that view.

Lastly, there are certain peculiarities in the aspect of the corona which seem only explicable on the theory above enunciated. Such are those radiations which are not at right angles to the Sun's limb; the phenomenon of loops of light in the corona with their concavity directed towards the Sun; the strange appearance resembling a hank of thread in disorder, seen by Arago in 1842; and other peculiarities too numerous to specify.

I know not of any phenomena which oppose themselves to the view here put forward, though I have carefully sought for such.

The spectroscopic analysis of the corona has not hitherto been altogether satisfactory, so that it may hardly be well to lay much stress upon it. It accords very satisfactorily, however, with the above theory. There would be a large quantity of reflected solar light in the corona, but there would also be much light from incandescent meteors, since those which came within a million or so of miles of the Sun would undoubtedly be raised to a white heat. Some of the meteors would, in all probability, be vapourised, and so bright lines might be seen in the coronal spectrum, as appears to have been the case during the total eclipse of last August. The observed association between meteors and comets suggests obvious considerations in explanation of the peculiarities said to characterise the spectrum of the corona. If the Great Comet of 1843 which passed within 65,000 miles of the Sun, has, like Tempel's comet, a train of meteoric bodies following in its track, these must be vapourised in the Sun's neighbourhood.

The contradictory evidence afforded by the polariscope is also obviously accounted for by the theory I have here advocated, even if it may not be said of itself, to force upon us the belief that the light of the corona is of that mixed kind which could scarcely result but in the way specified in that theory.

It would be desirable that measures should be adopted to insure the application of effective modes of observation during the very brief interval of total obscuration. I think this Society might with advantage appoint a committee to consider whether novel appliances and methods might not be employed to good purpose. The points I now proceed to touch on are so simple that some apology may, perhaps, be needed for bringing them under the notice of the Society; but if they should lead practised observers to make really important suggestions, my purpose will have been fulfilled.

In the first place, I would remark that observations specially directed to prove that the corona is a solar appendage would, in my opinion, be a complete waste of time and skill. It would be a misfortune — nay, it would even be in a sense discreditable — to astronomy, if the attention of observers should be directed to the solution of a question which has been practically solved during former eclipses. Unless the most obvious considerations of mathematics and optics are to be entirely neglected, the position of the corona as a solar appendage must be regarded as established, and all observations made with the object of confirming or disproving the matter, as simply futile.

But if we must travel over old paths, in order to establish that which is already evident, there are a few modes of observation which may be suggested as likely to give significant, however unnecessary, evidence.

If an observer were to confine his whole attention to the lunar disk during the eclipse, having a telescope with well-adjusted clock movement, and a field somewhat less than that of the full Moon, he would be able to recognise the following striking proofs of the real way in which the glare of the atmosphere varies during an eclipse. He would see, as the total phase approached, the atmospheric glare over the Moon's face gradually diminishing, and then what remained of actual glare from direct solar rays sweeping rapidly across the face of the Moon and leaving her disk relatively dark. But in a few moments the observer would be able (in favourable atmospheric circumstances) to recognise the spots on the lunar surface.

If an observer were to limit his attention to the Moon's disk during totality, keeping his eyes in darkness until the commencement of totality was announced by those around him, he would be certainly able to see the lunar spots, unless atmospheric conditions were very unfavourable indeed.

Attention might be directed to the shape and motions of the dark region of the sky surrounding the corona; and such observations would not be so complete a waste of time as those last considered, since it is evident that important information might be gathered from them respecting the height of the atmosphere. Such information would be in many respects more trustworthy than that which has been derived from the position, shape, and motions of the crepuscular curve.

But a mode of observation which I would advocate with great earnestness, is the simple application of telescopic power to determine, if possible, the structure of the corona. I have no doubt that this structure is continually changing; but most valuable information might be gained from a careful study of the position of the coronal beams at the time, and of those singularly complex hanks and streamers which have been already noticed by astronomers. The use of a telescope of low magnifying power, but first-rate definition, a comet eye-piece being employed, would be desirable in thus studying the corona. The telescope should be accurately driven by clock-work, and a dark iris-disk, if I may so describe an arrangement which would be the converse of an iris diaphragm, might be employed with advantage to hide the light of the prominences and chromosphere. If the field of view were several degrees in diameter, and the dark disk at the beginning of totality concealed a circular space extending a degree or so beyond the eclipsed Sun, the observer might first examine with great advantage the outer parts of the corona, and gradually extend his scrutiny to the very neighbourhood of the prominences. Supposing his eyes had been kept in darkness before totality began, he would be able to gain such an insight into the real structure of the corona as has never yet been obtained by astronomers.

As regards the spectroscopic and polariscopic analysis of the corona I shall say little. It would obviously be most desirable that Mr. Huggins, Mr. Lockyer, and those astronomers whose attention has been practically directed to researches of this sort, should give careful consideration to the question how the short interval of totality may best be employed, and that they should make their views public as early as possible. To one point, however, I shall venture to direct the attention of observers. It seems to me most important that every observer proposing to take part in applying such delicate light-tests to the corona, should prepare for the observations he is to make by keeping his eyes in darkness as nearly complete as possible for some time before totality commences; and further, where different parts of the corona are to be examined, the fainter parts should be first dealt with.

If the search for an intra-Mercurial planet is to be renewed with any chance of success, there can be little doubt that the telescopist must keep the corona, or at least its brightest portions, out of the field of view. A telescope specially constructed for the purpose, having a motion carrying the tube conically round a mean position might easily be devised; and with such an instrument one might conveniently sweep the Sun's neighbourhood all round the limits of the corona, for *Vulcan* and perhaps a train of attendant *Cyclopes*. But a telescope of low power, with a comet eye-piece, and a diaphragm concealing the brighter part of the corona, would probably be quite as effective.

For this class of observation, also, it would be very advan-

tageous that the eyes should be kept in darkness for some time before totality commenced.

Are observers to be found who, supposing the circumstances of the coming eclipse to be favourable, will be ready to forego the opportunity of witnessing one of the grandest of all natural phenomena, of watching the gathering shadows, of beholding the wonderful transformation of the face of nature, the weird and unearthly aspect of all things round them, and the strange beauty of the solar corona of glory, in order that they may devote all their observing energies during two short minutes to important, but severally uninteresting, phenomena? We know that, so far as the period of totality is concerned, such a sacrifice has already been made by De La Rue and Tennant, by Secchi, Janssen, Lieut. Herschel, Young, and a number of other lovers of science; but no observer has yet foregone the whole spectacle of a total eclipse for the sake of the dull, dry details of scientific observation.

Ephemeris of the Satellites of Uranus. By A. Marth, Esq.

Angles of Position at 8^h Greenwich Mean Time.

1870.	Ariel.	Umbriel.	Titania.	Oberon.
	°	°	°	°
Feb. 18	152	70	317	161
19	3	346	272	137
20	217	258	223	109
21	79	172	185	77
22	303	85	150	46
23	157	357	108	19
24	8	273	60	357
25	223	182	17	334
26	87	100	342	309
27	309	7	305	281
28	162	288	257	248
Mar. 1	13	193	211	218
2	229	114	174	193
3	93	18	139	170
4	314	301	95	148
5	167	204	46	122
6	18	128	7	92
7	236	30	332	60
8	100	314	292	31
9	320	216	242	7
10	172	140	200	345
11	24	42	165	321